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XLIX. *An Account of Observations made on the Mountain Schehallien for finding its Attraction. By the Rev. Nevil Maskelyne, B. D. F. R. S. and Astronomer Royal.*

Redde, July 6,
1775.

IN the year 1772, I presented the foregoing proposal, for measuring the attraction of some hill in this kingdom by astronomical observations, to the Royal Society; who, ever inclined to promote useful observations which may enlarge our views of nature, honoured it with their approbation. A committee was in consequence appointed, of which number I was one, to consider of a proper hill whereon to try the experiment, and to prepare every thing necessary for carrying the design into execution. The Society was already provided with a ten-feet zenith sector made by Mr. Sisson, furnished with an achromatic object glass, the principal instrument requisite for this experiment, the same which I took with me to St. Helena in the year 1761; which wanted nothing to make it an excellent instrument but to have the plumb-line made adjustable, so as to pass before and bisect a fine point at the centre of the instrument. This was ordered to be done, and a new wooden stand provided for it, capable of procuring a motion of the sector about a vertical axis, by means of which it could be more easily brought into the plane of the meridian,

ridian, or turned half round for repeating the observations with the plane of the instrument placed the contrary way, in order to find the error of the line of collimation. A large parallelopiped tent, $15\frac{1}{2}$ feet square and 17 feet high, was also provided for sheltering both the instrument and the observer who should use it, composed of joices of wood well framed together, and covered with painted canvas. The Society was likewise possessed of most of the other instruments requisite for this experiment; as an astronomical quadrant and transit instrument made by Mr. BIRD, and an astronomical clock by SHELTON, which had all been provided on occasion of the observations of the transit of Venus in 1761 or 1769. A theodolite of the best sort was wanting, a necessary instrument for obtaining the figure and dimensions of the hill. One of Mr. RAMSDEN's construction of 9 inches diameter, was thought the fittest for the purpose, on account of the excellence of the plan on which it was made, and the number of its adjustments, being capable of measuring angles for the most part to the exactness of a single minute. The other instruments prepared for this business were, two barometers of M. DE LUC's construction, made by Mr. NAIRNE; a common Gunter's chain; a roll of painted tape three poles long, having feet and inches marked upon it; two fir poles of 20 feet each, and four wooden stands, for supporting them when used in measuring the bases, and a brass standard of five feet for adjusting them. The poles and stands were provided on the spot.

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Although accounts had been received from various persons of several hills supposed proper for the intended purpose, some better and some worse authenticated; yet, in order to be sure of finding the best hill for the experiment, it was determined to send a person furnished with proper instruments, to make such observations on various hills in England and Scotland, as might enable us to choose the fittest for the purpose. Accordingly Mr. CHARLES MASON, who had been employed on several astronomical occasions by the Royal Society, was appointed to make a tour through the Highlands of Scotland in the summer of the year 1773, taking notice of the principal hills in England which lay in his route either in his going or in his return. It appeared from his observations, that scarce any hill was so well adapted to the purpose as our sanguine hopes had led us to expect; for either they were not high enough, or not sufficiently detached from other hills, or their greatest length fell in a wrong direction, too near the meridian, instead of lying nearly East and West, which is a circumstance requisite to make a hill of a given height afford the greatest effect of attraction. In particular, the hills on the confines of Yorkshire and Lancashire, mentioned in the foregoing proposal, were found not to answer the description that had been given of them. Fortunately, however, Perthshire afforded us a remarkable hill, nearly in the centre of Scotland, of sufficient height, tolerably detached from other hills, and considerably larger from East to West than from North to South, called by the people of the
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low country Maiden-pap, but by the neighbouring inhabitants, Schehallien; which, I have since been informed, signifies in the Erse language, Constant Storm: a name well adapted to the appearance which it so frequently exhibits to those who live near it, by the clouds and mists which usually crown its summit. It had, moreover, the advantage, by its steepness, of having but a small base from North to South; which circumstance, at the same time that it increases the effect of attraction, brings the two stations on the North and South sides of the hill, at which the sum of the two contrary attractions is to be found by the experiment, nearer together; so that the necessary allowance of the number of seconds, for the difference of latitude due to the measured horizontal distance of the two stations in the direction of the meridian, would be very small, and consequently not subject to sensible error from any probable uncertainty of the length of a degree of latitude in this parallel. For these reasons the mountain Schehallien was chosen, in preference to all others, for the scene of the intended operations; and it was concluded to make the experiment in the summer of the year 1774.

It was foreseen, that this experiment would be attended with considerable expence, and such as might easily have exceeded the common funds of the Royal Society, without some extraordinary assistance. The bounty of his Majesty, our Patron, happily removed this difficulty. At the humble request of the Society, his Majesty had been graciously pleased to grant a very ample

sum to their disposal, for defraying the expences of the observations of the late transit of Venus in 1769, as his Majesty had before done with respect to the former transit of Venus in 1761. Out of this benefaction, after all expences had been paid, there was a considerable remainder, and, the Society humbly requesting to know his Majesty's pleasure about the disposal of it, he was graciously pleased to direct them, to *lay it out in such manner as they thought proper, and was most agreeable to the end of their institution.* As this bounty of his Majesty had been originally granted for an astronomical purpose, the Society thought they could not dispose of it on any more important object, or in any manner more consistent with the intentions of their Royal Patron and Benefactor, than by expending it on this astronomical experiment of the attraction of a mountain, as what could hardly fail of throwing light on the principle of universal gravitation, and was likely to lead to new discoveries concerning the constitution of this earth which we inhabit, particularly with respect to the density of its internal parts.

The experiment being thus resolved upon, the next next thing to be done was to fix on a proper person to carry it into execution. Numerous and interesting as my literary engagements are at the Royal Observatory, I had no thoughts of undertaking this care and labour myself, till the Council of the Royal Society were pleased to do me the honour to think my assistance necessary to insure the success of so important and delicate an experiment. Their thinking so was a sufficient motive with me to
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encounter whatever difficulties and fatigues might attend operations carried on in so inconvenient and inclement a situation. But it was requisite I should also have his Majesty's permission for absenting myself so long from my duty at the Royal Observatory. This his Majesty was graciously pleased to grant; and to allow me to stay as long as I thought necessary, to complete *my very important observations*.

Such were the motives for undertaking this experiment, and the preparations made for putting it in execution. I am now to give an account of the operations themselves.

The quantity of attraction of the hill, the grand point to be determined, is measured by the deviation of the plumb-line from the perpendicular, occasioned by the attraction of the hill, or by the angle contained between the actual perpendicular and that which would have obtained if the hill had been away. The meridian zenith distances of fixed stars, near the zenith, taken with a zenith sector, being of all observations hitherto devised capable of the greatest accuracy, ought by all means to be made use of on this occasion: and it is evident, that the zenith instrument should be placed directly to the North or South of the centre of the hill, or nearly so. In observations taken in this manner, the zenith distances of the stars, or the apparent latitude of the station, will be found as they are affected by the attraction of the hill. If then we could by any means know what the zenith distances of the same stars, or what the latitude of the place would have been, if the hill had

been away, we should be able to decide upon the effect of attraction. This will be found, by repeating the observations of the stars at the East or West end of the hill, where the attraction of the hill, acting in the direction of the prime vertical, hath no effect on the plumb-line in the direction of the meridian, nor consequently on the apparent zenith distances of the stars; the differences of the zenith distances of the stars taken on the North or South side of the hill, and those observed at the East or West end of it, after allowing for the difference of latitude answering to the distance of the parallels of latitude passing through the two stations, will shew the quantity of the attraction at the North or South station. But the experiment may be made to more advantage on a hill like Schehallion, which is steep both on the North and South sides, by making the two observations of the stars on both sides; for the plumb-line being attracted contrary ways at the two stations, the apparent zenith distances of stars will be affected contrary ways; those which were increased at the one station being diminished at the other, and consequently their difference will be affected by the sum of the two contrary attractions of the hill. On the South side of the hill, the plumb-line being carried Northward at its lower extremity, will occasion the apparent zenith, which is in the direction of the plumb-line continued backwards, to be carried Southward, and consequently to approach the equator; and therefore, the latitude of the place will appear too small by the quantity of the attraction; the distance of the equator from
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the zenith being equal to the latitude of the place. The contrary happens on the North side of the hill; the lower extremity of the plumb-line being there carried Southward will occasion the apparent zenith to be carried Northward or from the equator; and the latitude of the place will appear too great by the quantity of the attraction. Thus the lesser latitude appearing too small by the attraction on the South side, and the greater latitude appearing too great by the attraction on the North side, the difference of the latitudes will appear too great by the sum of the two contrary attractions; if therefore there is an attraction of the hill, the difference of latitude by the celestial observations ought to come out greater than what answers to the distance of the two stations measured trigonometrically according to the length of a degree of latitude in that parallel, and the observed difference of latitude subtracted from the difference of latitude inferred from the terrestrial operations, will give the sum of the two contrary attractions of the hill. To ascertain the distance between the parallels of latitude passing through the two stations on contrary sides of the hill, a base must be measured in some level spot near the hill, and connected with the two stations by a chain of triangles, the direction of whose sides, with respect to the meridian, should be settled by astronomical observations.

If it be required, as it ought to be, not only to know the attraction of the hill, but also from thence the proportion of the density of the matter of the hill to the mean density of the earth; then a survey must be made

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of the hill to ascertain its dimensions and figure, from whence a calculation may be made, how much the hill ought to attract, if its density was equal to the mean density of the earth; it is evident, that the proportion of the actual attraction of the hill to that computed in this manner will be the proportion of the density of the hill to the mean density of the earth.

Thus there were three principal operations requisite to be formed. 1. To find by celestial observations the apparent difference of latitude between the two stations chosen on the North and South sides of the hill. 2. To find the distance between the parallels of latitude. 3. To determine the figure and dimensions of the hill.

I arrived at the hill of Schehallien on the last day of June, and found the observatory and instruments there, which had been brought down some time before from London to Perth on board a ship, and thence conveyed over land to the hill under the care of Mr. REUBEN BURROW, my late assistant at the Royal Observatory. The observatory was fixed half-way up the South side of the hill, as the place where the effect of the hill's attraction would be at the greatest, and it was placed in the like manner when it was afterwards removed to the North side. A circular wall was raised, five feet in diameter, and covered at top with a moveable conical roof for sheltering the astronomical quadrant; and a square tent was put up for receiving the transit instrument, all near to the observatory. A *bothie*, or temporary hut, was also made near it, for my residence, while I was attending

the astronomical observations on this side of the hill. I first put the sector, nearly in the meridian, by means of the variation compass; but, thro' the badness of the weather, which was almost continually cloudy or misty, I could not before the middle of July get a sufficient number of observations with the astronomical quadrant, to know the state of the clock, in order to draw a meridian line on the floor of the observatory, for setting the sector truly in the plane of the meridian. The first observations which I made with the sector, after it was put truly in the meridian, were on the 20th of July. Between this time and the end of the month, I observed the zenith distances of 34 stars, some to the North and some to the South of the zenith; and many of them several times over, having taken 76 observations in all, with the plane of the sector turned to the East. On the first of August, I turned the plane of the instrument about, to face the West, and set it in the meridian again, by means of the meridian line drawn on the floor the 26th of July, and secured by picquets driven into the ground; and between that and the 15th of the same month, I observed 39 stars including most of those taken in the former position of the instrument, and took 93 observations in all.

And here let me take notice of a method which I fell upon of verifying the position of the sector, with respect to the plane of the meridian, which, had I thought of it at first, would have saved me much trouble; and therefore I will now mention it, as it may be useful to future observers. It consists in observing the transits of two stars,

stars, differing considerably in declination from one another across the vertical wire of the sector, and comparing the observed difference of their transits with the known difference of their right ascensions. If they agree, it may be safely concluded, that the instrument is truly placed in the meridian. If not, by comparing the alteration that would be produced in the difference of the transits, by supposing the instrument out of the meridian, by any small quantity, as one degree or ten minutes, with the observed error, the deviation of the instrument from the meridian may be inferred. In this manner I found, that the instrument had been put very exactly in the meridian by means of the meridian line; the difference by the two methods coming out only $2\frac{1}{2}$ minutes of azimuth. As to the continuance of the instrument in the plane of the meridian, I had a constant proof of it by the same means, and likewise a farther security, which I did not fail to attend to, by noting the degree and minute which an index depending on the vertical axis of the instrument pointed out on a fixed azimuth circle. Being apprehensive of error in an instrument supported on a wooden frame, I frequently examined the parallelism of the fore arch to the back arch, by measuring their perpendicular distances at the two ends with a brass scale, whose vernier shewed the five hundredth part of an inch, and found it liable to variations of a minute or two, owing probably to the force used in setting the sector to different zenith distances, and the weakness of some screws at the top of the frame; which small error I corrected, till I found it liable

to continual returns: and I satisfied myself, that the plane of the sector never deviated above three minutes from the meridian in any of the observations taken on the South side of the hill, which, it is evident, could not in the least affect the observed zenith distances of stars. I hardly ever observed without examining the bisection of the point at the centre of the instrument, by the plumb-line; which was absolutely necessary, on account of the gradual changes of the wooden frame. My view in mentioning these minute circumstances is, to caution future observers, as well as to confirm my own observations. But whoever makes use of an instrument of this kind, supported on a wooden frame, will find the greatest attention necessary to attain the same degree of accuracy in his observations, as if his instrument were fixed to an immovable wall. In the mean time, by observations taken with the quadrant and transit instrument, I got a meridian line, and planted a pole to preserve it on the top of the hill, to the South of the instrument, and another at the foot of the same hill; from whence, by measuring off an equal distance to the East (as the South-west corner of the observatory lay to the East of the transit instrument) and setting up another pole, another meridian line was got, passing through the South-west corner of the Southern station of the observatory. The reason for making the meridian line pass through the South-west corner of the observatory rather than through the middle of it was, that this part of it had been taken when

the obfervatory had been ufed as an object in taking angles by the theodolite, in the furvey of the hill.

While I was engaged in thefe aftronomical obfervations, Mr. BURROW, attended by Mr. WILLIAM MENZIES, a land-furveyor living in the neighbourhood, who had been recommended by fome of the principal gentlemen of the country, as a proper perfon for this work, went out every day that the weather permitted, to take fections of the hill, and angles between feveral objects, for determining the figure and dimenfions of the hill. The method made ufe of was this, which was propofed by Mr. BURROW, and was well adapted to the purpofe. A number of ftation poles were fet up at convenient diftances all round the foot of Schehallien; but rather without its bafe, and chiefly on little eminences rifing from the foot of it, which formed a polygon of many fides, furrounding the hill; and when delineated on paper, fhew very nearly the fhape of its bafe. At each ftation, the angular pofition of two or more of the other ftations being obferved with the theodolite, and one fide being determined by means of a meafured bafe, all the other fides will be known. From thefe ftations, fections of the hill up to the top were taken in the following manner. The theodolite, being placed at any ftation, was pointed towards the hill; and a labourer was fent with a number of poles, which he was to plant in the ground truly upright, at regular diftances and in a vertical plane, according to fignals which he received from the perfon that flood at the theodolite, who alfo took the altitude of the foot of
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each pole, and likewise the horizontal angle contained between the plane of the section poles and the next station pole to the right or left. The theodolite was then removed, and planted directly over the centre of this station pole, which was removed for this purpose; and the horizontal angle taken between a pole now planted at the first station and each of the poles of the section. The horizontal distance of the two station poles being known, the horizontal distance of each of the section poles from the first station, and their respective perpendicular altitudes above it, or depth below it, will be given.

It is manifest, that these operations, when connected by angles with the two stations of the observatory and the meridian line, would at the same time give the shape and dimensions of the hill, and the distance of the parallels of latitude passing through the two stations of the observatory, as well as their respective elevations above the base of the hill. But errors being apt to accumulate in a long chain of triangles; to obviate this danger, as well as to produce a check on any great mistakes, that might happen to be made in reading off, or writing down, the angles, I caused a heap of stones, or *carn* as it is called by the people of the country, to be raised in a circular figure six feet high, at the highest point of the ridge of the hill, which is to the West of it, as a signal to be observed from the several angles of the polygon, and as a means of connecting the two stations of the observatory by a smaller number of triangles. Another *carn* towards the Eastern end of the ridge of the hill was

afterwards set up for the like purpose. I proposed to determine the distance of the two *carns* by connecting them by angles with a base, to be measured in a level spot in the vale below the hill, and then to make use of the said distance as a secondary base for determining the sides of the polygon, and the distance of the two stations of the observatory. Had the two *carns* been visible from the two stations of the observatory, two triangles would have sufficed for connecting the two stations together. But, notwithstanding that this was not the case, and that only the two *carns* were visible from one another, yet all the angles of these two triangles were measured by Mr. BURROW in the following method, suggested by himself. He went with the theodolite to the neighbouring hill on the South side of Schehallien, which runs parallel to it; and, by varying his situation, found a point whence the Western *carn* and Southern observatory appeared by the theodolite to be in one vertical plane, and removing the theodolite he planted a pole there. In like manner he planted another pole on the same hill, in a vertical plane with the Southern observatory and Eastern *carn*. Then returning to the observatory, he took the horizontal angle contained between the two poles, which it is evident is equal to its opposite angle, or that contained between the *carns*. And going to the West *carn*, he took the angle contained between the East *carn* and the pole planted on the opposite hill, in a line with the Southern observatory and West *carn*, which is the same with the angle between the East *carn* and Southern Observatory. And lastly, going to the East *carn*, he took the angle contained

tained between the Western *carn* and the pole placed on the opposite hill in a line with the East *carn* and Southern observatory, which is the same with the angle contained between the Western *carn* and Southern observatory. Thus were the three angles found of the triangle made by the Southern observatory and two *carns*. In the like manner were the angles of the triangle made by the Northern observatory and two *carns* found afterwards. And, as a proof that the angles of the two triangles were rightly determined, their sum in the first case differed from 180° by little more than two minutes; and in the second case by only half a minute.

Notwithstanding the advantages which attended this method of finding the distance of the two stations of the observatory, I thought it proper to make use also of the other method of doing the same thing by a small number of triangles carried directly across the hill, thinking it expedient, in a matter of such consequence, to rely on no single operation; but, as far as possible, to confirm every deduction by another found in an independent manner. I had caused two poles to be set as far up the hill of Schehallien as they could be placed; one as near the Western, and the other the Eastern *carn* as they could be, so as to be visible from the Southern station of the observatory: also two others in like manner visible from the North observatory; one of which was very near the East *carn*, and the other only 269 feet distant from the Westernmost of the two poles visible from the South observatory; so narrow was the ridge of the hill in that part, although it grew wider both to the West and East,

East, but much more towards the latter. With these four poles, the East *carn*, and the two stations of the observatory, five triangles were formed, connecting the two stations of the observatory, the relative situation of which to each other would be determined as soon as the length of any one of the sides of these triangles was known, either by comparing it with a base measured in the valley below, or with the distance of the two *carns* settled in that manner.

I had got sufficient observations of zenith distances of stars with the sector on the South side of the hill by the 15th of August; I prepared therefore for removing the observatory and instruments to the new station on the North side. This was a work of great labour and difficulty, as every thing was carried over the ridge of the hill on men's shoulders, and some of the packages were very weighty; it employed the labour of twelve men for a week, and was completed on the 26th. A large level area had been cut away, with great labour, here, in the side of the hill, for receiving the observatory, as had before been done on the South side of the hill. A new *boothie* was also erected, and places for holding the quadrant and transit instrument, as before, adjoining to the observatory.

The badness of the weather prevented me from beginning my observations with the sector till the 4th of September; but, that being a clear night, I had a fair opportunity of putting in practice the method of bringing the instrument into the meridian by the transits of the stars

stars across the plane of the sector, which was mentioned before. The sector being put up with its plane facing the West, and set near the meridian by the variation compass, allowing for the variation, I found, by the transit of α Draconis, on the North side of the zenith, compared with those of κ , ι and θ Cygni on the South side, that the instrument deviated $49\frac{1}{2}$ minutes to the West of the South in azimuth; which being corrected, by turning the instrument about on its vertical axis, towards the East, by the help of the divisions on the azimuth circle; I then found, by the transit of η Cephei, on the North side of the zenith compared with that of π Cygni on the South side, that the instrument deviated seven minutes to the East of the South in azimuth, which I corrected accordingly. And so near was it brought to the meridian in this manner, that by the most exact comparison of the transits of several stars on the 7th and 8th instant, it appeared to be only two minutes out of the meridian, and that to the East of the South; which small error I also attempted to correct; but the instrument rested one minute out of the position which I intended to give it, owing to the difficulty of turning it about to such great nicety, and so I let it remain.

It was indeed a most fortunate circumstance, that I thus got the instrument so near the meridian by the very first night's observations, those of September 4th; for the badness of the weather in the day prevented me from getting a meridian line by the Sun till the 15th. Had I therefore been obliged to wait for setting the instrument right by the Sun,
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I should have lost four good days observations, which were two-thirds of those I took on this side of the hill with the plane of the instrument turned to the West, and been retarded near three weeks in my observations; and, as the opportunities of weather fit for observing at all were but very rare, I might have been thereby thrown back into the winter, and defeated of making so complete a set of observations on the North side of the hill as I had got on the South side, whose correspondence would thereby have been rendered less perfect. I had the satisfaction, however, when I drew the meridian line on the floor of the observatory by the equal altitudes of the Sun taken on the 15th, to find it agree perfectly, even to the same minute, with the position of the instrument, as determined by the transits of the stars. But no one will doubt of the superior ease and readiness afforded by the latter method, in preference to the other.

On the 20th of September I completed the observations with the plane of the sector turned to the West, having observed 32 stars, and taken 68 observations in all. On the 22d, I turned it about with the plane to face the East, and set it again in the meridian, by putting it parallel to its former position, by means of the meridian line secured by marks made on picquets let into the ground perpendicularly below the plane of the instrument, before it was turned. Between this time and the 24th of October, I observed 37 stars, and took 100 observations in all, with the plane of the instrument facing the east:
and

and thus I completed my whole series of observations with the sector, having observed 43 different stars in all, on both sides of the hill, and taken 337 observations.

As a few observations, taken with so excellent an instrument as this zenith sector, would have been sufficient to determine the apparent difference of latitude of the two stations of the observatory, to a second or two; I am apprehensive I may be thought by many to have multiplied observations unnecessarily. However that may be, I apprehend, that doubling the observations in each station of the observatory, by taking them with the plane of the instrument alternately facing the East and West, will be allowed to be a proper step, as the line of collimation of the instrument is hereby separately determined at each station, and thereby all danger of any alteration happening in the same, in its removal from one side of the hill to the other, is intirely obviated. I had, indeed, all the reason in the world to think, that the sector was carried from one station to the other without the least accident; but still it was proper to guard against what was possible to happen.

But I had reasons also for multiplying the observations made in the same position of the instrument. It was important to demonstrate the exactness of the instrument from the near agreement of a number of observations taken with it, as its excellence was not to be intirely presumed, unless this proof could be shewn in its favour. Besides, it might be expected, that some unsteadiness or warp-

ing of the wooden stand on which it was supported, might affect the accuracy of the observations; or there might be variable and discordant refractions, even near the zenith, on the side of so steep a hill, more than are found in lower situations. Add to this, that when I began my observations on the South side of the hill, having a prospect of bad weather before me, and not knowing how few observations I might be able to get on either side of the hill, I thought it prudent to endeavour to observe most of the stars in the British catalogue, which came within the reach of the instrument, that I might be sure of being provided with observations of some at least of the same stars, which I might afterwards observe when I should be removed to the North side of the hill; where, after an interval of perhaps some months, many stars, that before passed the meridian in the night, would pass it in the day, and consequently be either invisible through the telescope of the sector, or more precarious of being seen.

Although a meridian line had been found by the transit instrument at the South observatory, whereby the relative situation of the two stations of the observatory, as well as of the other points of the hill, with respect to the meridian, might be determined; yet I judged it would be more satisfactory to confirm this by another meridian line drawn at the Northern observatory. This I found, as I had done the former, by setting the transit instrument to agree with the pole-star at the computed time of its passing the meridian, and confirmed it by comparing

the difference of the transits of the pole-star and of α Pegasi, α Andromedæ, and γ Pegasi, with their difference of right ascension, in the same manner by which I had put the sector in the plane of the meridian, and found it to agree with the former meridian line within two minutes.

It remains to give an account of the manner in which the two bases were measured; one in a level spot at the foot of the hill, to the Southward; and the other at the distance of about $2\frac{1}{2}$ miles from the hill to the North-west, in the plain of Rannock. I caused two measuring poles to be made of straight-grained well-seasoned fir, in the form of square tubes, 3 inches square and 20 feet long, and strengthened with square pieces within-side at several distances. These were carefully compared with the brass standard made by Mr. BIRD, the same which was used in the measure of the degree at Pennsylvania, immediately before they were applied to the measure of the bases, and the height of the thermometer noted at the time in order to make allowance for the expansion or contraction of the brass standard by heat or cold. Four wooden stands were provided for supporting the poles; each having a triangular base with three iron spikes beneath, at each of the angles. An upright pole, six feet high, rose from the middle of one side of the triangle, and two short braces were joined to it from the two ends of this side, and a long slant pole from the opposite angle. Two sliding arms were put upon the upright pole, capable of being raised or depressed, one above and the other below the

place where the flant pole was fastened to the upright pole, for supporting the measuring poles at a convenient height above the ground. In measuring the base, one end of a pole was supported on one of the stands, and the other end on another stand; and it was set horizontal by means of a spirit level laid on it about the middle, and by raising or depressing the arm on which it rested at one or the other end. The other pole was then, in like manner, supported on the two other stands truly level, and in the same vertical plane with the former pole, namely, that of the intended base, without regarding whether they were exactly of the same height, and with some small horizontal interval between their ends. This interval was measured by laying one leg of a brass rectangle, which was divided into inches and tenths, along one pole, while the other, or vertical leg, touched the end of the other pole: for it was not thought advisable, to bring the ends of the poles to touch exactly, as that would have taken up a great deal of time, and might have endangered the altering the position of the hindermost pole, if it should chance to receive any shock by laying down the foremost pole. It is evident, that the inches and tenths given by the divisions of the brass rectangle are to be added into one sum together with the poles, in computing the length of the base. When the foremost pole was truly placed, and the interval between them had been measured by the divided side of the brass rectangle, the hindermost pole was taken up, and the stands on which it had rested were advanced forwards, and the pole again laid
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on them, truly level, and in the true direction of the base. In order to set the poles continually in the proper direction of the base, the following method was used. The theodolite was first set up at one end of the base, and an upright pole at the other, and another in the middle, and a third was from time to time advanced to a little distance forward; and the measuring poles were sometimes placed in the proper direction by the eye, looking along the lengths of both poles together to the upright pole before them, and sometimes by the help of the theodolite. In this manner, about the middle of September, a base was measured by Mr. BURROW and Mr. MENZIES of 3012 feet, in the valley at the foot of the hill to the South-west; but not so accurately as this method is capable of, owing to the stands being very unsteady, through the looseness of the spikes in the feet and other faults, during the measuring the first quarter of the base, though they were mended before the mensuration of the remainder of it. The mensuration of another base of the length of 5897 feet, in the meadow of Rannock, about $2\frac{1}{2}$ miles to the South-west of the centre of the hill, which I attended myself, was performed with the greatest accuracy, according to the same method, on the 10th, 11th, and 12th of October, with new stands, more substantial and firm than the former.

The extreme badness of the weather no less retarded the operations of the survey than the celestial observations; for there was almost constant rain, mist, or high wind, to obstruct the use of the theodolite: indeed

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all the people of the country agreed, it was the worst season that had ever been known. So that it was not till the 20th of October that the sections had been carried all round the hill. Nor would this work have been so much forwarded as it was, had it not been for the use of an additional theodolite of the same construction, and by the same maker, as the former, which was lent me, upon my request, by the right honourable JAMES STUART MACKENZIE, lord privy seal for Scotland; who, having long cultivated a distinguished taste for astronomy, was pleased to honour the experiment of attraction with every assistance, which his interest or recommendation could procure. I am particularly to acknowledge the favour he conferred upon me by introducing me to the acquaintance of Sir ROBERT MENZIES, baronet, his brother-in-law, a gentleman conversant in mathematical and philosophical learning, who honoured me with his friendship during my residence in the country; and, besides many personal civilities shewn to myself, rendered many material assistances to the main purpose of carrying on the experiment. It is with pleasure also, that I acknowledge the civilities of all the neighbouring gentlemen, who often paid me visits on the hill, and gave me the fullest conviction that their country is with justice celebrated for its hospitality and attention to strangers. I was honoured also by visits from many learned gentlemen who came from a great distance; particularly the lord privy seal, Dr. WILSON, professor of astronomy at Glasgow, and his son,

son, and Dr. REID, professor of moral philosophy, and Mr. ANDERSON, professor of natural philosophy, also at GLASGOW, lord POLWARTH, Mr. RAMSAY, professor of natural history at Edinburgh, Mr. Commissioner MENZIES of the Customs at Edinburgh, Mr. COPLAND and Mr. PLAYFORE, of the university of Aberdeen, the rev. Mr. BRICE, and my esteemed friend Col. ROY, who had been my companion in the journey as far as Edinburgh. So great a noise had the attempt of this uncommon experiment made in the country, and so many friends did it meet with interested in the success of it!

The use of the two theodolites at once, as mentioned above, much forwarded the completing of the sections all the month of October; Mr. MENZIES observing the bearings at one station with one theodolite, while Mr. BURROW observed the altitudes or depressions with the other theodolite at the other station; and the labourer, who used to plant the poles in the hill, taking only one pole with him, and fixing it up at one place to be observed at both theodolites, and then removing it to the next station for the like purpose. Notwithstanding which, the weather grew at length so bad, by the early coming in of frost and snow in the beginning of November, when the survey was near completed, as to render it impossible to do any thing more that season. It became therefore necessary to finish this astronomical campaign, leaving the theodolite in the care of Mr. MENZIES, to complete what little remained to be done the next season.

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I have thus described the plan which was adopted for the operations on Schehallien, and the manner in which it was carried into execution; it only remains to give the result of computations made upon those operations for deducing the effect of the attraction of the mountain. The operations themselves at large shall be communicated another opportunity.

I had caused the arch of the sector to be divided by fine points, according to a new and arbitrary division adapted to the method of continual bisection. One-eighth part of the radius of the instrument was found by three bisections, and applied as a chord to the arch from the middle on each side, intercepting each way

7° 9' 59,917. These spaces were each divided by points into 128 parts, by continual bisection; therefore one division will contain

3° 21' 56,1854. Hence the number of degrees and minutes answering to any number of divisions may easily be found. Twenty-four additional parts were also set off taken from the former, and added at the 128th division on each side, to fill up the whole extent of the arch, which thus consisted of 152 divisions on each side of the centre, answering to an angle of

8° 30' 37,4, which was therefore the greatest angle the instrument was capable of measuring. To find the value of the parts of the micrometer in seconds, I measured the distance of the points on the limb, by five at a time, by means of the plumb-line, in parts of the micrometer
from

from 0 to 128 on each side of the middle or point marked 0 upon the arch. By a mean of all these mea-

tures, one division of the arch or $3' 2''$, 562 came out equal to four revolutions and 34,8272 parts of the micrometer, 41 of which make one revolution; and there-

fore one part is equal to $1,0137545''$, and 41 or one re-

volution is equal to $41,5639345''$. Hence the value of any number of revolutions and parts of the micrometer may be easily found. At all observations of the same star, whether on the North or South side of the hill, I brought the same point of the arch, namely, that which agreed nearest with the zenith distance of the star, under the plumb-line, so that the difference of the apparent zenith distances of the same star on contrary sides of the hill is given in parts of the micrometer, and has no reference to the divisions of the instrument, whether they be equal or unequal; and, the parts of the micrometer screw being perfectly equal, as I had formerly satisfied myself by measuring the interval of two given points on the arch with different parts of the screw, that difference of apparent zenith distances may be perfectly relied on, as far as depends upon the instrument, provided the bisection of the star by the wire in the telescope, and that of the point on the arch by the plumb-line, were accurately performed. As the plane of the instrument was placed both East and West, at both stations of the observatory, the difference of the latitude of the two stations may be found

as well from the observations made in one position of the instrument as the other. If the instrument had suffered no change by being carried over the hill, that is to say, if the line of collimation was not altered thereby, the results should come out equally true from the observations taken in both positions of the instrument. On the contrary, if the line of collimation should, by any means, have suffered any alteration between the observations made at the two stations, this would cause the difference of latitude to appear too small, by the observations made in one position of the instrument, by the quantity of the alteration, and as much too great, in the other position of the instrument. But still the mean between the two results, deduced from the observations taken in the two different positions of the instrument, would give the true difference of latitude; and that equally, whether the line of collimation had suffered any change or not. Therefore this will be the best method of comparing the observations together, and I shall take a mean of all the results, deduced from the observations taken in each position of the instrument separately, and then a mean of those means for the true difference of latitude. By single observations of ten stars; *viz.* β , α , and η Cassiopeæ, and ι , η , β , 39, 45, 46, and 53 Draconis, made on both sides of the hill, with the plane of the sector facing the West, after making the proper allowance for precession, aberration, and deviation, and semi-annual solar nutation of the earth's axis (see my tables annexed to my Observations made at the Royal Observatory), the apparent difference

ference of latitude between the two stations of the observatory, comes out $54,1$, $54,7$, $54,0$, $55,4$, $55,0$, $55,0$, $52,2$, $54,0$, $54,3$, $53,1$, respectively; the mean of all which is $54,2$; the greatest difference of any one result from the mean being only $2''$. In like manner, by single observations of as many stars; *viz.* β and α Cassiopeæ; ϵ Urfæ majoris; β , 39 , 46 , 0 , 49 , and 53 , Draconis; and 23 Cygni; made on both sides of the hill, with the plane of the sector facing the East; after making all the allowances as before, the apparent difference of latitude comes out $54,5$, $52,3$, $56,8$, $53,5$, $54,5$, $57,2$, $56,1$, $55,3$, $54,1$, $55,1$, respectively; the mean of all which is $55''$; the greatest difference of any one result from the mean being $2''$. The two means $54,2$ and $55,0$ differ only $0,8$, which should argue only an alteration of $0,4$ in the line of collimation; but this is too small a quantity to be depended on; and therefore it is most probable, that the state of the instrument remained unvaried. However, whether it did or no, the mean of the two means, or $54,6$, is to be esteemed the apparent difference of latitude between the two stations of the observatory, and, when compared with the difference of latitude which should result from the trigonometrical measures, will give the sum of the two contrary attractions of the hill.

It muſt be owned, that this point will be ſettled with rather more certainty when all the obſervations made with the ſector, which exceed 300, ſhall have been computed; but, as from the agreement of theſe reſults together, as well as from the ſmall differences that are uſually found in obſervations made within a few days of one another, one may preſume, that the reſult from the whole will not differ materially from that deduced above from 40 obſervations, I thought I had better take this opportunity of gratifying the impatience of the Society in preſenting them with theſe my firſt computations before their ſummer reſceſs, than delay giving them any account at all of this experiment, till I had leiſure to complete the whole of my calculations.

I am now to ſhew, what the diſtance is between the parallels of latitude paſſing through the two ſtations of the obſervatory in feet, according to the trigonometrical menſuration; and thence, what the difference of latitude ought to have been, if the hill had been away, or had exerted no ſenſible attraction. This depends on the enumeration of ſeveral particulars.

The length of the baſe meaſured in the meadow of Rannoch was 5897,119 feet, according to the ſtate of the braſs ſtandard when the thermometer was at 40°; but, to reduce it to anſwer to the ſtate of the braſs ſtandard in the heat of 62°, we muſt ſubtract 16,721 feet; we ſhould alſo ſubtract farther 0,327, for the diminution which the braſs ſtandard has ſuffered by wear, and there remains 5880,071 feet for the true length of
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the base in Rannoch. See Phil. Transf. vol. LVIII. p. 313. 324. and 326. Hence, with the help of the angles taken with the theodolite at the ends of the base in Rannoch, and at the West *carn*, the horizontal distance between the East and West *carns* comes out 4047,4 feet. Nearly the same result comes out from the base measured on the South side of the hill, though with less exactness; this, when all corrections are made, is 3011,684 feet, whence the distance of the two *carns* should come out 4058 feet, or about ten feet longer than results from the base in Rannoch. But I prefer the deduction from the base in Rannoch as most to be depended on. Hence, by the calculation of the two triangles formed by the two *carns* and the two stations of the observatory, the distance between the parallels of latitude passing through the two stations comes out 4364,4 feet, which, according to M. BOUGUER's table of the length of a degree in this latitude of $56^{\circ} 40'$, at the rate of 101,64 English feet to one second, answers to an arc of the meridian of $42,94''$. The other series of triangles carried across the hill, gives the same distance of the parallels only 10 feet less, and consequently the arc of the meridian only $\frac{1}{10}$ th of a second less. Thus the difference of latitude found by the astronomical observations, comes out greater than the difference of latitude answering to the distance of the parallels, the former being $54,6''$, the latter only $42,94''$. The difference $11,6$ is to be attributed to the sum of the two contrary attractions of the hill.

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The attraction of the hill, computed in a rough manner, on supposition of its density being equal to the mean density of the earth, and the force of attraction being inversely as the squares of the distances, comes out about double this. Whence it should follow, that the density of the hill is about half the mean density of the earth. But this point cannot be properly settled till the figure and dimensions of the hill have been calculated from the survey, and thence the attraction of the hill, found from the calculation of several separate parts of it, into which it is to be divided, which will be a work of much time and labour; the result of which, will be communicated at some future opportunity.

Having thus come to a happy end of this experiment, we may now consider several consequences flowing from it, tending to illustrate some important questions in natural philosophy.

1. It appears from this experiment, that the mountain Schehallien exerts a sensible attraction; therefore, from the rules of philosophizing, we are to conclude, that every mountain, and indeed every particle of the earth, is endued with the same property, in proportion to its quantity of matter.

2. The law of the variation of this force, in the inverse *ratio* of the squares of the distances, as laid down by Sir ISAAC NEWTON, is also confirmed by this experiment. For, if the force of attraction of the hill had been only to that of the earth, as the matter in the hill to that of the earth, and had not been greatly increased by the near ap-

proach to its centre, the attraction thereof must have been wholly insensible. But now, by only supposing the mean density of the earth to be double to that of the hill, which seems very probable from other considerations, the attraction of the hill will be reconciled to the general law of the variation of attraction in the inverse duplicate *ratio* of the distances, as deduced by Sir ISAAC NEWTON from the comparison of the motion of the heavenly bodies with the force of gravity at the surface of the earth; and the analogy of nature will be preserved.

3. We may now, therefore, be allowed to admit this law; and to acknowledge, that the mean density of the earth is at least double of that at the surface, and consequently, that the density of the internal parts of the earth is much greater than near the surface. Hence also, the whole quantity of matter in the earth will be at least as great again as if it had been all composed of matter of the same density with that at the surface; or will be about four or five times as great as if it were all composed of water. The idea thus afforded us, from this experiment, of the great density of the internal parts of the earth, is totally contrary to the hypothesis of some naturalists, who suppose the earth to be only a great hollow shell of matter; supporting itself from the property of an arch, with an immense vacuity in the midst of it. But, were that the case, the attraction of mountains, and even smaller inequalities in the earth's surface, would be very great, contrary to experiment, and would affect the
measures

measures of the degrees of the meridian much more than we find they do; and the variation of gravity in different latitudes in going from the equator to the poles, as found by pendulums, would not be near so regular as it has been found by experiment to be.

4. The density of the superficial parts of the earth, being, however, sufficient to produce sensible deflections in the plumb-lines of astronomical instruments, will thereby cause apparent inequalities in the mensurations of degrees in the meridian; and therefore it becomes a matter of great importance to chuse those places for measuring degrees, where the irregular attractions of the elevated parts may be small, or in some measure compensate one another; or else it will be necessary to make allowance for their effects, which cannot but be a work of great difficulty, and perhaps liable to great uncertainty.

After all, it is to be wished, that other experiments of the like kind with this were made in various places, attended with different circumstances. We seldom acquire full satisfaction from a single experiment on any subject. Some may doubt, whether the density of the matter near the surface of the earth may not be subject to considerable variation; though perhaps, taking large masses together, the density may be more uniform than is commonly imagined, except in hills that have been volcanos. The mountain Schehallien, however, bears not any appearance of having ever been in that state; it being extremely solid and dense, and seemingly composed of an intire rock.

rock. New observations on the attraction of other hills, would tend to procure us satisfaction in these points. But whatever experiments of this kind be made hereafter, let it be always gratefully remembered, that the world is indebted for the first satisfactory one to the learned zeal of the Royal Society, supported by the munificence of GEORGE THE THIRD.

Observations of Stars made with the zenith sector on the mountain
Schehallien, for discovering its attraction.

Result of Observations on the South-side of Schehallien.											
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the East.				Limb of the Sector turned to the West.					
α Cassio- peæ	24S	July 28. S 1.35,4::				Aug. 4. S 2,7,1	7. 5,5				
"	1S					Aug. 7. N 10,4	10. 12,1	15. 12,7			
β Urfæ Major.	16N					Aug. 8. N 2.23,1					
γ	31S	July 29. N 2.16,9				Aug. 8. N 2.5,7:					
"	9N	July 28. N 2.23,6									
ζ	10S	July 28. N 1.11,5									
"	111S	July 22. N 19,8	28. 22,6								
Draconis	55N	July 26. N 2.26,0	28. 26,0			Aug. 4. N 2.14,0					
θ	45N	July 26. N 4,2				Aug. 4. S 7,5					
"	96N	July 25. N 6,2	26. 5,4			Aug. 2. S 6,5	4. 5,4				
β	75S	July 21. N 1.37,4	24. 37,0	25. 36,1		Aug. 1. N 1.27,4	2. 24,7:	4. 24,4	5. 26,9	6. 26,4	15. 28,7
ξ	4N	July 20. N 3.4,5	21. 4,3	25. 5,4	26. 6,4	Aug. 1. S 9,2	15. 7,9				
γ	92S	July 20. N 1.32,0:	25. 32,3			Aug. 5. N 1.20,7					
39	36N	July 24. N 19,1	26. 19,5	29. 19,8		Aug. 1. N 7,9	4. 11,3	15. 11,1			

		Result of Observations on the South-side of Schehallien.											
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the East.						Limb of the Sector turned to the West.					
45 Draco- nis.	4N	July 21. N 1,0	29. 2,8					Aug. 1. S 9,4	4. 11,2	15. 5,6			
46	24S	July 24. N 23,9	26. 24,7					Aug. 4. N 13,6	10. 16,4 :	15. 16,6			
o	44N	July 21. S 4,2	25. 2,7	26. 2,2	28. 2,0	29. 1,0		Aug. 1. S 12,7	4. 12,4				
48	15N							Aug. 11 N 2,5,1	15. 6,0				
49	24S	July 29. N 2.28,0						Aug. 1. N 2.16,4	4. 17,6				
53	3S	July 26. S 22,2	28. 21,5					Aug. 1. S 32,0	4. 31,5	15. 28,4			
54	12N	July 29. S 18,4						Aug. 15. S 25,5					
α Cygni	66S	July 21. S 0,1	26. N 0,3										
'	97S	July 21. N 2.36,0	28. 35,0	29. 35,8	31. 36,5			Aug. 1. N 2.23,5	4. 22,8				
θ	124S	July 26. S 19,6	31. 17,5					Aug. 1. S 31,4					
23	5N	July 24. N 8,5	29. 8,7	31. 11,0				Aug. 1. S 2,7	4. 2,4	7. 1,5	15. N 1,1		
33	14S	July 24. N 1.0,2	26. 0,5	29. 1,3	31. 3,1			Aug. 1. N 30,4	4. 30,7	7. 31,6	15. 34,9		
η Cephei	77N	July 23. S 5,5	26. 2,4	29. 1,2	31. 1,6			Aug. 1. S 12,5	4. 11,2	7. 10,5	10. 9,8	15. 7,5	
α	89N	July 23. S 25,0	26. 22,0	28. 20,8	29. 21,0	31. 20,0		Aug. 1. S 33,9	4. 32,8	7. 32,8	10. 31,7	15. 28,5	
π Cygni	116S	July 29. N 12,8						Aug. 7. 0,0 :	10. N 1,9				

		Result of Observations on the South-tide of Schehallien.									
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the East.					Limb of the Sector turned to the West.				
10 Cephei	61N						Aug. 4. N 28,8				
12	44N						Aug. 12. N 21,9				
13 "	20S	July 26. N 34,0	29. 34,2				Aug. 7. N 24,7	10. 27,1	15. 28,3		
14	4N	July 26. N 2,38,5					Aug. 4. N 2,27,7	10 31,5			
15 "	37N						Aug. 12. S 24,3				
21 ζ	8N	July 29. S 1,4,7	31. 2,9				Aug. 4. S 1 14,6	10. 11,5	15. 11,0		
	13S	July 23. S 27,8	26. 26,7				Aug. 4. S 36,0	10. 33,8			
27 δ	11N	July 26. S 37,0	30. 36,9				Aug. 4. S 1,6,7	7. 6,7	9. 6,0	10. 5,5	15. 3,3
	149N						Aug. 9. N 1,3,5				
1 Caffio- peæ	27N	July 29. N 2,29,9	31. 30,3				Aug. 4. N 2,20,7	15. 22,9			
2	26N	July 31. S 13,0					Aug. 4. S 24,9	9. 23,5	15. 21,4		
5 "	13N						Aug. 10. N 16,2	15. 17,5			
7 ε	8S						Aug. 10. S 2,14,4	15. 13,6			
β	22N	July 26. N 39,2					Aug. 4. N 29,0	7. 29,5	15. 31,9		

Result of Observations on the North-side of Schehallen.											
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the West.				Limb of the Sector turned to the East.					
α Caffio-peæ.	24S	Sept. 8. S 3,9,0	15. 7,4			Oft. 2. S 2,27,5	3. 25,3	5. 26,8	24. 22,3		
"	1S	Sept. 7. S 33,8	8. 33,5	19. 30,3		Oft. 2. S 13,4	3. 11,8	9. 11,0	24. 7,2		
β Urfæ Major.	16N					Oft. 6. N 1,3,0					
γ	31S					Oft. 14. N 29,0	15. 25,5	18. 26,5	22. 26,4		
"	9N					Oft. 3. N 35,5	4. 35,6	15. 30,0	17. 33,1		
ζ	10S	Sept. 15. S 24,2				Oft. 16. S 19,6	22. 22,0				
"	111S					Oft. 17. S 1,6,3	18. 6,6	22. 6,9			
δ Draconis	55N	Sept. 20, N 37,6				Oft. 17. N 1,6,1	22. 1,4				
θ	45N					Oft. 17. S 1,13,7	23. 16,4				
"	96N	Sept. 7. S 1,18,0	20. 19,4			Oft. 23. S 1,12,3					
β	75S	Sept. 7. N 17,3	15. 17,5			Oft. 7. N 28,8	17. 27,2	23. 26,1			
ξ	4N	Sept. 15. N 1,29,5				Oft. 23. N 1,39,9					
γ	92S	Sept. 18. N 13,6	20. 13,4								
39	36N	Sept. 7. S 36,8	20. 38,9			Oft. 3. S 25,4	17. 26,5				
45	4N	Sept. 7. S 1,14,0				Oft. 17. S 1,1,1	23. 2,0				

Result of Observations on the North-side of Schehallien.										
Names of the Stars.	Point on the limb.	Limb of the Sector turned to the West.				Limb of the Sector turned to the East.				
46 Draco- nis.	24S	Sept. 7. S 33,0				Oct. 3. S 20,9	17. 18,7	23 19,4		
°	44N	Sept. 18. S 1.16,5				Oct. 3. S 1.4,8	18. 3,3	22. 3,6		
49	24S	Sept. 20 N 1.12,2				Oct. 3. N 1.25,5	22. 26,5	23. 26,3		
53	3S	Sept. 7. S 1.35,0				Oct. 3. S 1.21,6	17. 20,6			
54	12N					Oct. 17. S 1.17,5	22. 18,0			
α Cygni	66S					Oct. 18. S 40,0				
'	97S	Sept. 7. N 1.20,0	18. 20,7	20. 20,5		Oct. 3. N 1.36,0	15. 35,9	18. 36,7		
θ	124S	Sept. 7. S 1.34,6	20 33,7			Oct. 6. S 1.17,0	22. 17,6			
23	5N	Sept. 7. S 1.5,9	18. 3,9	20. 3,0		Oct. 3. S 29,3	15. 28,3	18. 28,6		
33	14S	Sept. 7. S 13,1	18. 10,3	20. 10,8		Oct. 3. N 4,0	6. 5,8	7. 5,8		
η Cephei	77N	Sept. 7. S 1.12,5	8. 13,1	18. 9,3	20. 10,6	Oct. 3. S 36,9	6. 35,1	7. 35,9		
α	89N	Sept. 7. S 1.33,6	8. 35,0	20. 31,9		Oct. 3. S 1.13,5	6. 12,0	23. 9,7		
π Cygni	116S	Sept. 7. S 1.1,1	8. 1,4			Oct. 6. S 21,5	9. 21,4			
10 Cephei	61N	Sept. 20. S 10,0								
13 "	20S	Sept. 7. S 17,5	18. 14,9			Oct. 3. N 1,5	7. 1,7	9. 3,5		

Result of Observations on the North-side of Schehallien.										
Names of the Stars	Point on the limb.	Limb of the Sector turned to the West.				Limb of the Sector turned to the East.				
14 Cephei	4N					O& 9. N 2,9,8				
21 ζ	8N	Sept. 7. S 2.16,0	18. 11,7	20. 11,2:		O& 3. S 1.35,6	6. 35,3	9. 33,5		
'	13S	Sept. 7. S 1.38,0	8. 37,3			O& 3. S 1.17,5	9. 15,0	23. 13,3		
27 δ	11N	Sept. 8. S 2.8,0				O& 3. S 1.30,6	6. 26,5	9. 25,5	22. 22,6	23. 22,6 24. 23,4
'	149N	Sept 8. N 1,7	15. 3,8							
1 Caffio- peæ.	27N	Sept. 7. N 1.17,4	13. 21,2	15. 20,4		O& 3. N 1.39,0	6. 39,9	9. 2.0,0		
2	26N	Sept. 7. S 1.27,0	8. 26,0	15. 23,5		O& 3. S 1.4,5	6. 5,4	9. 3,9		
5 τ	13N	Sept. 7. S 29,4	8. 28,1	13. 26,4		O& 2. S 7,8	3. 7,8	9. 5,1		
7 ε	8S	Sept. 7. S 3.18,7	8. 18,5	15. 18,3		O& 3. N 1.38,0	6. 36,0	9. 40,5	24. 2.4,6	
β	22N	Sept. 7. S 14,0	8. 13,6	13. 11,7	15. 11,7	O& 3. N 6,9	6. 7,9	9. 8,9		

N. B. The figures fet down under the days of the month are the revolutions and parts of the micrometer, by which the star is North or South of the division shewn in the second column. The revolution, though not repeated, is supposed to continue the same as in the first ob-

observation of the star. One part of the micrometer is = $1,0137545''$, and one revolution or 41 parts is = $41,56393''$. 128 divisions of the arc have the chord = $\frac{1}{8}$ th of the radius.